THURSDAY, JUNE 5, 1884

THE ORIGIN OF THE CRYSTALLINE SCHISTS

Untersuchungen über die Entstehung der Altkrystallinischen Schiefergesteine mit besonderer Bezugnahme auf das Sächsische Granulitgebirge, &c.," von Dr. Johannes Lehmann. (Bonn: Hochgürtel, 1884.)

NO problem in modern geology stands out with such prominence as the origin of that remarkable group of rocks to which the name of the Crystalline Schists has been given, and to none in recent years has so large a share of the literature of the science been devoted. The question is attacked on all sides. By some observers its solution is sought in laborious investigations of the hilly and mountainous regions where these rocks hold their empire among the grander solitudes of nature. By others the question is studied in the quiet of their own libraries or laboratories with all the resources of modern chemistry and microscopy. Great progress has indeed been made in these various ways. Regarding certain aspects of the problem a general agreement has been arrived at; but there are others as to which the difficulties remain as persistently obstructive as ever.

One of the most important contributions to the study of this fascinating subject has just appeared in the form of a handsome quarto volume, with a large atlas of plates, by Dr. J. Lehmann. This author has enjoyed exceptional opportunities of qualifying himself for the task he has now undertaken. For nine years, as a member of the Geological Survey of Saxony, he was engaged in the investigation of the classical granulite-region of that kingdom, of which he surveyed the southern and rather larger half, while his colleague, Mr. E. Dathe, investigated the northern part. The maps of this Survey are accompanied with explanatory pamphlets, among which Dr. Lehmann's detailed local observations have already been published. But it was desirable to present a generalised description of the whole region and to discuss the bearings of the observations upon theoretical questions. He originally proposed to undertake this task in association with Mr. Dathe; but his transference to Bonn as Privat-docent in Mineralogy and Geology, and the removal of his friend to the Prussian Geological Survey, having prevented the intended cooperation of the two observers, Dr. Lehmann has himself worked up the mass of materials collected during his long course of work in the field. To enlarge the scope of his inquiries and obtain additional data for comparison he has recently extended his investigations into the Erzgebirge, Fichtelgebirge, and the mountains of the Bavarian and Bohemian frontier. And he now offers what we may hope is only a first instalment of his results.

Naumann, whose early account will always be cited as a model of careful observation and accurate description, regarded the granulite of Saxony as an eruptive rock—an opinion in which he has still modern followers, including our author himself. He recognised a fact which seems in more recent times to have been lost sight of, that a gradation can be traced from the more highly crystalline condition of the granulite centre, through successive zones of mica-schist, and other schists, into the older sedimentary rocks of the surrounding districts. These schistose rocks have in more recent times been classed as "Archæan," and as such they appear on the maps of the Saxon Geological Survey, Dr. Lehmann having himself accepted this view in his earlier published descriptions. But more extended study of the subject has induced him to abandon the idea of the existence of any Archæan nucleus and to return to a modification of the original conception of Naumann. How he has been led to this conclusion it is the object of his volume and atlas to show.

Under the deep cover of post-Tertiary deposits, the granulite tract of Saxony forms a central ellipse round which zones of various schistose rocks are grouped, that pass outwards into the normal clay-slates of that part of Germany. These slates on the south-eastern margin are unconformably overlaid by Silurian and Carboniferous rocks. On the north-west side a conformable sequence is traceable from the schists and slates upwards into Cambrian and Lower Silurian rocks, which are precisely like those of the adjacent countries. Instead of being Archæan masses, Dr. Lehmann concludes that the whole of the crystalline schists within the granulite area are metamorphosed Palæozoic sediments. They may be originally of Silurian or Cambrian age, and their metamorphism probably took place during the crumpling and upheaval of the area, that is, later than the Devonian and older than the Carboniferous period.

Towards the establishment of this conclusion the author brings forward a vast mass of detail, which he skifully arranges so that its bearings upon theoretical questions may be clearly seen. At the same time he endeavours to separate rigidly what is demonstrable fact from what is mere inference, and in this lies one of the most valuable features of his memoir. He has collected such a body of evidence as will give a new impetus to the study of metamorphism, while at the same time it provides abundant new and suggestive material for the prosecution of this study. He justly cites the Saxon granulite area as a classic example of the occurrence and origin of metamorphic schists where a complete gradation can be followed from unaltered or little altered sediments into wholly crystalline foliated masses. In this progressive intensity of metamorphism the most notable fact is the corresponding advance in the development of mica. Over and above all local diversities of mineral character, there is a constant augmentation in the quantity and size of the mica-folia. At the same time the muscovite, which is alone present in the outer parts of the area, is replaced further inwards by biotite. Nor is this change confined to the peripheral schists; it extends into the granulite of the centre. Such a rearrangement of the mineral constituents of the rocks cannot be explained by any hypothesis of an eruptive granitic mass. Like so many other concurrent facts, it points to the effects of the molecular movements of the original rocks, sedimentary or other, under the strain to which they were subjected during the process of crumpling and upheaval. Where these movements have been greatest, there the accompanying metamorphism has been most intense, and, as one prominent indication of this change, there is the

most abundant development of biotite. Every student of the crystalline schists can furnish parallel examples to those cited by Dr. Lehmann where, on the zigzag puckerings that form so striking a feature among these rocks, a copious growth of biotite or some other mica has taken place.

Among the metamorphosed rocks of the Saxon region some of the most instructive are bands of conglomerate interstratified among the schists. The sedimentary origin of these zones is of course unquestionable, and so obvious that the alteration to which they have been exposed furnishes a kind of sample of the initial stages of change which are so often lost where the clastic materials are of a less prominent and obdurate character. The pebbles of granite, quartz, &c., have been deformed and more or less altered, so that sometimes they seem to shade off into the surrounding matrix. The latter has become a crystalline micaceous mass by which the pebbles are wrapped round. These conglomerate bands have thus been converted into half-crystalline gneiss-like schists.

A specially important part of the memoir deals with Gabbros and Amphibolites. These rocks, as members of the series of crystalline schists, have long been a puzzle to those who have studied them in the field. That they are metamorphic rocks, and not rocks of original chemical precipitation, has been inferred from their association with masses whose original sedimentary origin admits of no doubt. But even those who have held this view have hesitated as to the nature of the original masses out of which they have come. Many years ago Jukes suggested that hornblende-rocks and hornblende-schists might represent ancient lavas and tuffs interstratified with the sediments which are now schists and quartzites. And it seems probable that this opinion is essentially correct. Dr. Lehmann goes into great detail regarding the structure of the diallage and hornblendic rocks of the granulite tract. His study of them leads him to conclude that the gabbro is an eruptive rock, younger than the granulite but older than the granite, which has been involved in the general metamorphism and has consequently assumed schistose modifications. "I know no rock," he adds, "which illustrates so well the effect of mechanical pressure upon a solid rock as the gabbro of the Saxon granulite tract. While other rocks leave us in doubt as to their original condition, the gabbro supplies us with every stage from the beginning to the end of the metamorphism." These conclusions possess at present a special interest in relation to the crystalline schists of this country. The Geological Survey, in the course of an investigation of the schists of the north of Scotland, has recently come independently to similar deductions with regard to the diorites and amphibolites of Aberdeenshire and Banffshire. Among the schists of that region there occur extensive masses of diorite. This rock presents sometimes the typical composition and structure of a diorite, and under the microscope appears as one of the most beautiful examples of a thoroughly crystalline granitoid mass. It behaves in the field as an eruptive rock, which has risen generally parallel with, but also transgressive across, the bedding of the contiguous schists. It is obviously from these characters a mass that has been intruded into the clay-slates, knotted-schists, and other schists of the district. Being traversed by veins and bosses of granite, its protrusion was obviously earlier than that of some at least of the granite. Further examination of it, however, shows that in many places it presents a remarkable parallelism in the arrangement of its crystalline constituents. Sometimes this is shown by the orientation of the feldspars in one definite direction. In other places the feldspar and hornblende are drawn out into more or less distinct bands. Further stages of change reveal the feldspar segregated into an almost pure labradorite rock, while the hornblende appears as a felted mass of hornblende-schist. Some of these schistose aggregates are of exquisite beauty. Over wide tracts biotite has been abundantly developed in the diorite, and sometimes also numerous and large kernels of garnet. It is observable that the direction of the foliation of the diorite coincides with that of the surrounding schists. There seems no reason to doubt that, as these Scottish schists are metamorphosed Lower Silurian sediments, the diorites and amphibolite-schists represent Palæozoic eruptive rocks that have participated in the general metamorphism. Dr. Lehmann recognises, in the Mica-schist and Phyllite groups, hornblende-schists which he thinks may have been embedded masses of diabase that have been more or less altered.

His general conclusions are thus summed up:—"I cannot regard the metamorphic schists (mica-schists, gneisses, &c.) as 'Archæan' formations. It does not appear to me to be established that genuine gneisses anywhere came out of pre-Cambrian sediments. The production of such rocks as mica-schist, &c., belongs to the time of mountain-upheaval, and in actual fact has involved formations of far younger age than the Cambrian. In the Saxon granulite region it is later than the Devonian period." He draws a distinction between what he considers to be "true gneisses" and other rocks to which the general name of gneiss has been applied. He restricts the appellation to the foliated forms of granite. This foliated or true gneissic structure he believes to be more or less due to metamorphism by stretching, seldom wholly original, so that many gneisses may be called metamorphic; only, the original rock was not a sediment but a mass that consolidated from fusion (Erstarrungsgestein). We fear that a theoretical distinction of this kind will involve all kinds of practical difficulties in its general application.

Reference must be made to the atlas that accompanies the memoir. It contains 28 plates, on which are placed no fewer than 159 photographs of thin sections of the rocks described in the text. Unlike the usual illustrations of this kind, these photographs represent the objects of the natural size, or less, or at most only slightly magnified. They are not microscopic studies, but show the actual structure of the rocks as seen by the naked eye or with a weak lens. It is impossible to speak too highly of the success with which they have been produced. With their aid we are rendered in some measure independent of the actual specimens, and can follow with pleasure and satisfaction the detailed descriptions of the author. No such wealth of accurate illustrations has yet been furnished for the study of this important series of rocks. Dr. Lehmann, however, is, we hope, only on the threshold of his inquiries. A vast domain lies before him where the problems are many and the qualified observers are but few. He has done excellent service by presenting in this compendious form such

an array of facts as a trained geologist can gather in the field, and by boldly announcing the conclusions to which the study of these facts has led him. But much more may be made of them than he has yet given us. And we trust he may be encouraged to continue the investigation he has so well begun. ARCH. GEIKIE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Marine Biological Association

WILL you allow me space to ask all naturalists and lovers of science who intend to become members of the above Association to send their names and subscriptions *without delay* to Mr. Frank Crisp, 6, Old Jewry, London, E.C. The subscription is one guinea annually, or fifteen guineas for life membership. It is highly desirable that intending members should at once enrol themselves, since the first meeting of the Association for the election of officers and council for the year 1884-85, and for the ratification of by-laws, will be held in London at the end of this month, when Prof. Huxley will be nominated as President. Donations, whether large or small, are earnestly solicited. Those who are interested in the natural history of marine plants and animals, and who foresee the immense help to this study which a well-equipped laboratory will afford, are begged not only to give some pecuniary aid to the present enterprise, but to constitute themselves agents of the Association and to do their best to persuade others to contribute to the fund required for building the first biological laboratory on the English coast. It is only by hearty and earnest support of this kind that our object can be realised.

I may add that several naturalists have contributed each 100*l.* to the Association, others 25*l.*, and others less, according to their means and their sympathy with our object. Of the 10,000*l.* required, we have not yet obtained half.

E. RAY LANKESTER,
Secretary (*ad interim*)

11, Wellington Mansions, North Bank, N.W.

The Equatorial Coudé of the Paris Observatory

IN continuation of my first letter I now proceed to answer M. Lœwy's second letter, as published in your issue of May 15 (p. 52).

M. Lœwy has not, as I said in that former letter, raised a single objection which had not already been anticipated and discussed with the exception of one which I shall treat of further on. The several points in this letter I shall dispose of very shortly.

1. As to the dialyte construction, I have to reply that that particular method of achromatising the objective is not an *essential* feature of this instrument. Whether it be adopted or not is in fact much a question of *cost*. If the purchaser desires to get the largest possible aperture at least expense, then I would make it a dialyte, for, notwithstanding all M. Lœwy says, good work can be and has been done with dialytes. If, however, the most perfect instrument is desired, I would dispense with the dialyte construction, and achromatise the object-glass in the ordinary way, which is quite as applicable to my construction of equatorial as is the dialyte. If I mistake not, the celebrated observer M. Dembowski observed for many years with a dialyte, and spoke highly of it; he says: "*L'achromatisme est excellent.*" Again, the present director of the observatory, for whom the first of these instruments is to be made, has worked already with dialytes, and he would not be likely to recommend this construction if his experience agreed with M. Lœwy's. I desire to notice just one further point in this part of M. Lœwy's letter, as it is another example of how his own words (unintentionally, no doubt) confirm my statements. He says (speaking of the limited field of view of dialytes): "But, in order to turn the difficulty, he" (Mr. G.) "suggests that since the field of view becomes smaller as the instrument becomes larger, we may content ourselves with observing at a central point." I never said this; my words were: "The definition at the edge of the field, however, is not so good as in the ordinary form, but this would not be of so much consequence in large instruments, as the field in such cases is never of great extent." And M. Lœwy himself corroborates this for me when he says: "For the observation of comets I have such an eyepiece, which magnifies fifty times, and has a field of view such that I can observe a degree (*i.e.* with the 12″ equatorial *coudé*); for a telescope of 27 inches we might have such an eyepiece with a field of 24 minutes." Thus I have a distinct corroboration from M. Lœwy of what I said above.

2. Writing on the matter of stability, M. Lœwy curiously mixes up stability and accuracy of movement. Now while I claim that I can and will obtain greater stability in my form than exists in M. Lœwy's, I do not claim accuracy of movement, but on this point I propose to say very little at present for several reasons. In the first place, it would hardly be possible to discuss this and put it in an intelligible form to your readers without a careful drawing; secondly, the well-known stability of the instruments which have emanated from my workshops are quite sufficient guarantee that this point is not one likely to be neglected in any of my work; and thirdly, I find it utterly impossible to understand the sentences of M. Lœwy's paper bearing on this point, and if I, though familiar with the proposed construction, fail to understand them, I am hopeless of serving any useful purpose by discussing them in your columns, particularly as few of your readers have ever seen the design of the instrument referred to. M. Lœwy talks of "all movements of transmission being broken at right angles." I do not know what he means, but he omits to tell your readers that, according to my design, in the larger sizes I propose that all movements be effected by two hydraulic cylinders the valves of which are within reach of the observer while sitting in his chair; so that, without more physical exertion than is necessary to open a water-tap, he has full command of all the movements of the great instrument, a pair of vertical scales on the walls of his study giving the approximate position of instrument in R and declination, an arrangement eminently calculated to reduce the work of the observer.

3. Lastly, as to its want of universality. This is distinctly stated in my paper as a disadvantage of my form; but when M. Lœwy asserts that "it is based on a principle which no astronomer can admit, viz. that it is superfluous to observe the greater part of the northern heavens," it is evident that M. Lœwy has here gone too far, since that portion of the heavens within 20° of the Pole is only about 6 per cent. of the northern hemisphere. Ask any practical astronomer possessing a moderate-sized equatorial how many hours out of the total number of hours which he has worked in the year has his instrument been pointed to objects within 20° of the Pole, and, with the exception of a few who apply themselves to special work, the great majority will give a reply which will show how very little will be lost by the fact that this instrument cannot command that portion of the heavens. I have myself put this question to many, and with the result above mentioned. On this point I cannot do better perhaps than give an extract from a letter I have just received from the director of one of our public observatories:—"Instruments of large aperture are rarely if ever used for observations where extreme accuracy of measurements is required, such as annual parallax, nor for searching for nor observing comets, except to search along a known track for an expected periodical comet. This your instrument could do well. There is hardly an instrument in existence which is equally well adapted to all kinds of observations. The circumpolar zone of about 20° may be explored by other instruments, but for almost every kind of *systematic* work the remainder of the visible heavens will give plenty to do." The foregoing would be a sufficient answer to a question which M. Lœwy has put directly to me.

He says:—"Permit me to ask Mr. Grubb how he is going to study that part of the heavens which lies between 20° from the zenith and the Pole."

To any one who has seen my paper it will be evident that this point, which in M. Lœwy's letter is put forward as a discovery of his own, was already fully dealt with by me. I said: "The instrument commands the heavens from east to west and from south horizon to about 20° beyond zenith." And again: "As regards this instrument (equatorial *coudé*) I would observe that it possibly possesses an advantage over my form in being absolutely universal."